Peer-review report of

Bélanger, J.J., Snook, D.W., Ahmed, M.R. & Adam-Troian, J. (2026). The radicalization syndrome: Reciprocal dynamics of obsessive passion and deliberate engagement. *advances.in/psychology*, 1, e912056. https://doi.org/10.56296/aip00067

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Round 1

Editor

Date: 2026-03-15

Dear Authors,

Thank you for submitting your manuscript, “No Passive Extremists: Active Exposure to Radical Content Mediates the Link between Obsessive Passion and Political Violence,” for consideration in our special issue, Advances in the Psychology of Violent Extremism. We have received feedback from two experts with significant expertise in this area, and we appreciate their detailed comments. As an editorial standard, we both assessed the manuscript before reading the evaluations. These evaluations, in conjunction with our own editorial review, have informed our decision.

We all see several positive aspects of your work. Reviewer 1 notes that it assesses the “holy grail of violent extremism research” and that “the attempt to distinguish between different forms of exposure to radical content is, in principle, worthwhile.” Similarly, Reviewer 2 notes that “the topic is timely and relevant to the journal”. We would also like to note that the use of samples from various political and religious groups, as well as the combination of correlational and longitudinal data, is a clear strength.

While both reviewers offer various suggestions for improvement, one suggests rejecting the paper and the other a major revision. Because we see value in the data, we want to give you the opportunity to address their concerns and ours. **We therefore invite you to submit a revision of your paper. Please note, however, that major revisions to the framing of the paper, and especially to the analytical strategy, will be required. A successful revision must address all the points mentioned. Please inform us whether you are motivated or not to conduct such an extensive revision or if you would like to discuss any specific points beforehand.**

The reviewers devoted considerable effort to providing thoughtful feedback; while we ask that you respond to each of their points, we will not reiterate them in full here. Instead, we wish to highlight the overarching issues that we and the reviewers consider critical for your revision.

Both reviewers raise conceptual doubts regarding the core concept of obsessive passion. They question whether the construct is theoretically distinct from related terms that are already well established in the field. While we do not request that you provide new data on this, we do ask that you critically review literature demonstrating that it is factorially distinct from the constructs noted by the reviewers and that it possesses discriminant validity (e.g., correlations with them are not excessively high). We also request that you provide factor analyses showing that obsessive passion, harmonious passion, and commitment are factorially distinct in the present research.

Reviewer 2 further questions the face validity of your scale, noting that the items seem to tap into constructs other than what is typically regarded as passion.

Similar concerns apply to your proposed mediators and dependent variable. Some exposure subscales are so highly correlated (for example, the correlation between active seeking and active action reaches .93 and .96 in two studies) that it is almost guaranteed they are not statistically distinct. Unfortunately, these high correlations extend to the associations between several exposure scales and the proposed dependent variable. Therefore, please conduct a confirmatory factor analysis to determine whether the three exposure subscales and the support for violence scale truly represent four distinct factors. Most likely, the fit will be poor. In that event, please run exploratory factor analyses to merge certain scales and construct alternative models.

Related to this, we have conceptual concerns regarding the treatment of the mediator and the outcome. The manuscript's central finding is that only "active action" mediates the link between obsessive passion and support for political violence. However, "active action" includes highly proximal forms of politically confrontational or extremist engagement, such as meeting extremists, attending protests, and seeking weapons- or bomb-related information. Conceptually, as Reviewer 1 notes, this is not a clean exposure variable. It is already a form of ideological engagement that sits very close to the outcome. In practice, it looks less like "exposure to radical content" and more like early-stage radical behavioral involvement. This creates a serious construct-overlap problem.

As it stands, the EXPO-12 framework is treated as if all four subscales are exposure types, but active action is qualitatively different from passive online/offline exposure and even from active seeking. The theoretical model would be much cleaner if the manuscript explicitly argued that active action is not just exposure, but a participatory immersion mechanism. At present that distinction is blurred.

More broadly, one branch of the mediator is behavioral participation, another is active information seeking, another is incidental online exposure, and another is incidental community exposure. These are not simply different dimensions of the same construct. They may reflect different theoretical domains altogether, such as information acquisition, social embeddedness, behavioral participation, and environmental exposure. The paper does not yet provide enough conceptual work to justify treating these as parallel variants of a single overarching mediator family. In particular, as noted above, active action seems to belong more to radical engagement than to exposure, which weakens the model's coherence.

The mediation predictions and tests also raise major questions. First, as Reviewer 2 notes, alternative models are theoretically very plausible. For instance, "support for violence predicts engagement in non-normative actions, which in turn reinforces beliefs." This obvious alternative model is neither theoretically outlined as an alternative nor tested in the manuscript. Testing two

potentially competing or reciprocal pathways could prove to be an interesting objective of a revised paper. Furthermore, the introduction needs to explain why passive online and offline exposure might theoretically be expected to be predicted by obsessive passion. Currently, this is unclear. Why should an incidental occurrence be directionally predicted by a psychological tendency? There are likely valid arguments for this, but they must be explicitly presented and anchored in the existing literature.

Unfortunately, the current test of mediation is underwhelming. Study 1 is limited by its correlational nature. Demonstrating these associations correlationally across several samples lends credence to the possible existence of such a mediation, but it does not serve as a definitive test, and you must also test the alternative plausible model suggested by Reviewer 2. This is precisely why Study 2 is so critical. Regrettably, it misses the opportunity to test for mediation using the interesting prospective data because it relies on a growth model rather than an RI-CLPM. As both reviewers highlight, a growth model is entirely ill-suited for testing mediation. As both reviewers state, the manuscript currently claims causality in a way that is not meaningfully supported by any of the analyses. Please provide an RI-CLPM instead. Note that the results of this new model, whether supportive of the initial predictions or not, will not negatively influence our editorial decision.

We would also ask you to be more careful in distinguishing support for political violence, radicalization, and violent extremism. At several points, the manuscript appears to blur these constructs or treat them as if they are interchangeable. Similarly, the dependent variable throughout is support for political violence, but in several places the writing drifts toward claims about “escalating violent intentions,” “pathways into extremism,” or “behavioral involvement over time.” Because the outcome is still a self-report attitude scale, these broader claims need to be scaled back or more carefully qualified.

Please also ensure that your new analyses account for non-normal distributions, which are, of course, common in this field. If statistical assumptions are violated, you could address this via transformations, bootstrapping of full models, or the use of other modeling approaches as Reviewer 1 suggests.

Finally, both reviewers note that your reliance on MTurk samples raises significant concerns. To be fair, most of the studies they cite do not rely on the CloudResearch platform, which has implemented various strategies to ensure data quality. However, we need to know in more detail how data quality was ensured in the present case. For instance, what prior approval rate was required, and what specific attention checks did your studies include?

In addition, we think the manuscript needs to engage more critically with the comparability of the groups it analyzes. The paper compares Democrats, Republicans, environmentalists, Muslims, and far-right participants. These are not equivalent ideological categories. Some are partisan identities, one is a religious identity, one is an issue-based activist identity, and one is a far-right self-placement sample. The paper treats them as if they are parallel ideological groups, but they differ

dramatically in social structure, normative status, motivational basis, and likely meaning of “political violence.” This creates a serious comparability problem.

In particular, “Muslims” are not analogous to “Democrats” or “environmentalists.” Muslim identity is not necessarily a political ideology. The theoretical rationale for including Muslims alongside overtly ideological groups is underdeveloped. A reader could reasonably ask whether the same passion construct, exposure measures, and violence scale have the same meaning across these populations.

A related concern arises with the operationalization of ideological passion in the Muslim sample. In Study 1D, the construct labeled “ideological passion” appears to measure attachment to Islam rather than ideological passion in the same sense used for the political groups in the other samples. Many of the items used to measure this construct (e.g., “Islam is important to me,” “I like Islam,” “Islam is well integrated into my life,” “Islam allows me to live a variety of experiences,” “I spend a lot of time thinking about Islam”) appear to capture religious identification, religiosity, or personal attachment to religion, rather than ideological passion in a political or movement-related sense.

This raises two related concerns. First, the construct validity of the measure is unclear. The scale appears to be adapted from the dualistic model of passion, but in this context the items primarily assess religious importance and integration into the self, which may simply reflect religiosity. In contrast, ideological passion in the political samples seems to refer to commitment to political causes, movements, or ideological projects. It is therefore not obvious that the same psychological construct is being measured across groups.

Second, this creates a comparability problem across samples. Religious identity (“Islam”) is not directly analogous to political ideology (e.g., partisan identity, environmental activism, or far-right ideology). For many respondents, Islam may represent a cultural, moral, or spiritual identity rather than a political ideology. Consequently, items such as “Islam is important to me” or “I like Islam” may reflect normative religious commitment rather than ideological passion. If so, the meaning of the passion construct differs across samples, which complicates interpretation of cross-group comparisons.

Relatedly, given the very different ideological samples, the manuscript should at least discuss whether the passion, exposure, and political violence measures function equivalently across groups. Even if full invariance testing is not feasible, addressing this issue explicitly would strengthen the paper.

In addition to these larger overarching points, please address the following specific issues in your revision.

INTRODUCTION

You measure and control for commitment, but the introduction does not clarify its purpose. What role does it play in your theoretical model?

METHODS

Please make the AMOS and PROCESS syntax or code needed to replicate your findings available via OSF, including the code for the power simulation.

Additionally, please indicate the countries of origin for participants in Study 1C, Study 1D, and Study 2.

Providing a table that shows detailed demographics for each of the samples analyzed in your paper would be highly beneficial.

When describing your measures, provide example items for each construct. As both reviewers stated, it is currently very difficult to understand how concepts were assessed. For instance, for support for political violence we consulted the cited paper, which measures behavioral intentions, but it remained unclear how this measure was adapted for your study.

Throughout the manuscript, please provide 95% CIs instead of SEs.

Avoid formulations such as “x was not related to y.” Because you did not directly test the null hypothesis, please state “x was not statistically significantly related to y” instead (you are mostly accurate in your language but there are some exceptions).

Furthermore, present standardized betas in all model figures with 95% CIs for each association (instead of SEs) and exact three-digit p-values.

Please also indicate how many bootstrapped resamples were used for all relevant tests.

Finally, you will need to test for selective attrition in Study 2 based on your key variables. The far-right longitudinal study drops from 600 at Time 1 to 347 by Time 3 (which are separated by just a few months). That is a substantial attrition rate. The manuscript notes that this is consistent with personality research, but that is not sufficient. What matters is whether the attrition was selective. If more extreme, less stable, or less engaged respondents dropped out, the slope estimates may be

biased. We therefore think the paper would benefit from a more direct analysis and discussion of attrition diagnostics and missingness assumptions.

Please provide a detailed description of whether the samples you used were utilized in other published or unpublished papers. Our submission system identified a potential match published in 2022.

STYLISTIC

Please ensure that all scale endpoints are italicized. Additionally, italicize all Latin statistical letters, including N, but do not italicize Greek letters.

In summary, we do see the kernel of an interesting manuscript in your submission, but its full potential has not been realized in its current form. We therefore invite you to submit a revised version that addresses the points outlined above. Please submit your revision by May 15. Should you require additional time or wish to discuss how best to address these comments, please do not hesitate to contact us. We look forward to receiving your revised work.

Best regards,

Jonas R. Kunst & Milan Obaidi

Reviewer 1

See attached file. Thank you very much!

Attachment: Reviewer 1 review (Round 1).pdf — appended at the end of this report.

Reviewer 2

The manuscript AIP469 titled “No Passive Extremist: Active Exposure to Radical Content Mediated the Link between Obsessive Passion and Political Violence” provides insights into the relationship between obsessive passion, exposure to radical content online and offline, and support for political violence. This relationship is examined in two studies among U.S. participants: one cross-sectional with four variations and one short-term longitudinal. The study found that only active active exposure to radical content mediated the link between obsessive passion and support for political violence.

The topic is timely and relevant to the journal. However, several conceptual, measurement, and modeling limitations substantially weaken the strength of the conclusions. In particular, key constructs are insufficiently delineated or their role is overstated, mediation claims are not adequately supported by the analytic approach, and findings are overinterpreted.

Introduction

1. Conceptual ambiguity and potential redundancy of obsessive passion (OP)

The manuscript introduces obsessive passion (OP) as a novel driver of radicalization; however, both its conceptualization and measurement suggest substantial overlap with existing constructs capturing ideological identity and ideological intensity. In particular, OP appears to reflect the degree to which a political ideology or cause is central to the self-concept, emotionally salient, and motivationally compelling - features that are already well documented in the literatures on ideological identity, partisan strength, and affective partisanship (e.g., Huddy, Mason, & Aarøe, 2015; Mason, 2018). As such, it remains unclear whether OP represents a theoretically distinct construct or primarily re-labels established concepts under a different name.

This concern is reinforced by inspection of the item content. The OP and Ideological Commitment measures both assess the intensity, centrality, and salience of involvement in the environmental cause. The distinction between the two constructs therefore appears to be one of degree rather than kind, with OP representing a more extreme or emphatic version of commitment rather than a qualitatively distinct psychological process. Notably, the ideological commitment scale itself includes an item explicitly referring to passion, further blurring the boundary between the constructs.

Moreover, neither scale includes items that capture defining features of obsession as conceptualized in the broader passion literature, such as ego-dystonic conflict, distress, or persistence despite negative consequences. Instead, the items primarily index deep engagement and identity centrality. Consequently, OP may reflect heightened ideological identity or intensity rather than a distinct obsessive mechanism.

This overlap is particularly consequential given that ideological identity strength and ideological intensity are already known to predict political engagement, norm-challenging behavior, and tolerance for political violence. Without clear theoretical differentiation and evidence of discriminant validity - both conceptually and empirically (e.g., exploratory factor-analytic separation) - the added explanatory value of OP remains uncertain.

2. Overemphasis on “active action” and underestimation of other exposure pathways

The manuscript positions active action as the primary mediator linking OP to support for political violence, while downplaying the roles of passive exposure and active seeking. This framing appears overly restrictive.

Passive exposure may indirectly foster radicalization by normalizing extremist ideas or motivating subsequent active seeking. Likewise, active seeking plausibly precedes active action. Given that all forms of exposure are strongly intercorrelated and related to both OP and support for political violence, claims that only active action matters appear overstated.

It is also unclear why passive exposure and active seeking were excluded from the longitudinal study, as their inclusion would have allowed tests of temporal ordering among exposure pathways.

3. Unclear theoretical ordering of behavior and beliefs

The assumed temporal ordering, that is, behavior (active action) as mediator and beliefs (support for political violence) as outcom, is not theoretically justified. An equally plausible alternative is that support for violence predicts engagement in non-normative actions, which in turn reinforces beliefs. Reciprocal processes are also plausible. The authors should acknowledge these alternative causal structures and formally test them.

Methods and Results

1. Insufficient reporting of psychometric properties of the OP scale

Key psychometric properties of the OP scale are not reported for the present sample, including factor structure and evidence of discriminant validity. Several items across the ideological commitment and passion subscales appear highly overlapping (e.g., “The Democratic Party is a passion for me”), raising concerns about construct separability.

External validity of the scale is also not established. Additionally, the manuscript uses the terms *passion* and *obsession* interchangeably, despite their distinct conceptual meanings, further contributing to construct ambiguity.

2. Missing details regarding measurements

The manuscript does not clearly report item stems or response scales for several measures. For example, it is unclear how the response format (from stronly agree to strongly disagree) corresponds to items such as “Doing risky or illegal actions to help the Democratic Party win the next elections.”. Full reporting of stems and scale anchors is necessary for transparency and replication.

3. Lack of decomposition of between-person and within-person effects

The longitudinal analyses rely on a parallel latent growth curve model (LGCM). In such models, intercepts represent between-person differences in initial levels, and slopes represent between-person differences in rates of change. Consequently, slope-to-slope associations reflect stable

individual differences in trajectories rather than within-person change net of between-person variance.

Although LGCMs describe within-person change descriptively, associations among slopes capture between-person co-development (e.g., individuals who increase more in X also increase more in Y), not within-person dynamic processes (e.g., whether changes in X predict subsequent changes in Y). The current analyses therefore do not separate within-person fluctuations from between-person differences.

4. Mediation is not adequately tested

Mediation is inherently a temporal process that requires (a) temporal precedence, (b) change-based modeling, and (c) control for prior levels to rule out stability or reverse causation. These conditions are not met in the current analyses. All growth factors are estimated over the same time interval, and no lagged or change-score structure is specified. As a result, the model does not establish temporal precedence, and the reported indirect effects reflect statistical decomposition of co-development rather than evidence of mediation.

This concern is compounded by the relatively poor fit of the longitudinal model (e.g., RMSEA). To strengthen the conclusions, the authors should test alternative theoretically plausible longitudinal models in both studies, including: (1) Ideology - support for violence - exposure/engagement; (2) Support for violence - ideology - exposure/engagement.

More generally, the data could be reanalyzed using models that explicitly test temporal precedence and separate within- and between-person variance (e.g., autoregressive path models, latent change score models, or random-intercept cross-lagged panel models).

5. Unclear use of control variables

It is unclear whether ideological commitment and harmonious passion were controlled for across all studies and time points. The authors indicate that these variables were controlled only at Time 1, but do not explain why. If these constructs were measured repeatedly, controlling them at all time points would be helpful to support claims regarding the unique role of OP.

6. Model fit concerns

The longitudinal model exhibits poor fit according to RMSEA. Conventional cutoff (RMSEA ≤ .06) is not met. It is also unclear why TLI was reported instead of SRMR, particularly given the poor RMSEA. These model fit issues should be addressed.

7. Very short longitudinal time frame

The rationale for one-month intervals between waves is unclear. Specifically, it is unclear whether meaningful changes in obsessive passion, exposure/engagement, and support for political violence can be expected over such short intervals in an adult sample. The short-term nature of the longitudinal study should be added to its desctiptions consistently throught the manuscript and interpreted cautiously.

8. Data quality concerns

Exclusive reliance on Mechanical Turk raises concerns in light of evidence that data collected on MTurk cannot be trusted. These issues should be addressed in the methods and discussion sections.

https://link.springer.com/article/10.3758/s13428-025-02852-7

https://journals.plos.org/plosone/article?id=10.1371/journal.pone.0279720

https://pubmed.ncbi.nlm.nih.gov/34590289/

https://journals.sagepub.com/doi/full/10.1177/0149206320969787

9. Code is missing in the OSF repository.

Discussion

1. Overinterpretation of longitudinal findings

Slope-to-slope associations from a parallel LGCM are interpreted as evidence of a mediational process. However, as noted above, such models estimate between-person differences in trajectories and do not establish temporal ordering or within-person mediation. The strongest defensible conclusion is that individuals who increased more in obsessive passion also tended to increase more in active action, which was associated with greater increases in support for political violence. All claims regarding mediation, temporal sequencing, or psychological mechanisms should therefore be tempered.

2. Overstated contributions

Claims that radicalization is “driven by obsessive passion” are not sufficiently supported, given conceptual overlap with existing constructs and the modeling limitations noted above. Similarly, the manuscript’s claim to move beyond “static or short-term associations” is overstated, as the longitudinal study also captures only cross-sectional and short-term associations across one-month interval.

3. Problematic intervention implications

The proposed intervention strategies (e.g., monitoring protest attendance, redirecting activism) raise feasibility and ethical concerns. Monitoring citizens’ political behavior is invasive, costly, and ethically questionable. Moreover, protest is a fundamental democratic activity and a legitimate form

of non-violent ideological expression. Framing protest as something to be “redirected” toward “constructive forms such as civic organizing, advocacy, or volunteering” risks conflating normative political participation with violent extremism.

Additionally, the authors acknowledge that normative activism may itself act as a gateway to radicalization, which undermines the proposed interventions or strategies. As currently articulated, the practical implications lack clarity and actionable guidance.

Round 2

Editor

Date: 2026-07-09

Dear Authors,

Thank you for submitting your revised manuscript, “Deliberate Engagement, Not Incidental Exposure: Obsessive Passion and Support for Political Violence,” for further consideration in our special issue, Advances in the Psychology of Violent Extremism. As in the previous round, we both assessed the revision before reading the evaluations, and our decision is informed by the reviewers’ comments in conjunction with our own editorial review.

We all appreciate the effort that you put into the manuscript, and we, the editors, really appreciate the transparency and nuance with which you present your analyses, including the reporting of inadmissible solutions that many authors would have left in a drawer. The reviewers also found much to like. The reviewer who was previously recommending rejecting the paper now suggests accepting it. They state: “Long story short, I was thoroughly impressed by the revised manuscript—the extent of the revision in terms of improving the analysis, presentation, and interpretation is outstanding.” Reviewer 2 remains more critical, while also acknowledging “several analyses that were requested, including an improved longitudinal modelling.”

We thus believe that your paper is on a strong trajectory, but we concur with the reviewers that some additional work is needed to bolster the robustness of your work. We therefore invite you to submit a further revision of your paper. Please address all points raised by the reviewers and by us, and this time, alongside the response letter that outlines the changes, provide a document with tracked changes so that we can easily judge the changes.

As before, the reviewers devoted considerable effort to providing thoughtful feedback; while we ask that you respond to each of their points, we will not repeat every point here. Reviewer 1 notes some minor issues that we ask you to consider. Reviewer 2 provides a set of more substantial issues that need to be addressed. Below we highlight the issues that we consider critical for your revision, together with several points arising from our own reading of the revised analyses.

We agree with Reviewer 2 that a longitudinal confirmatory factor analysis and tests of measurement invariance across the three waves are needed to support the RI-CLPM. The within-person components of these models are only interpretable if the constructs retain the same measurement structure over time. Please provide these analyses and report at least configural,

metric, and scalar invariance (or partial invariance where full invariance fails), and discuss any consequences for the within-person estimates.

Please also report the key fit statistics, including the currently missing indices such as RMSEA and SRMR from Tables S2 and S3, in the main text rather than only in the supplement.

We also need to understand the process by which the equality constraints on the lagged paths were determined. Were these set a priori according to some recommendations? If so, please explain this in the manuscript and reference these recommendations. Alternatively, if you did so stepwise via model comparisons, this process also needs to be detailed, including the fit of the unconstrained models.

In Study 2, you write: “The reciprocal path from engagement to passion was the larger of the two.” This needs to be tested, for example by setting the two cross-lagged paths equal and evaluating the resulting change in model fit. We would extend the same requirement to the passion–violence model, where it is even more consequential. The “reverse-only” characterization currently rests on the observation that one cross-lagged path is statistically significant (B = .096) while the other is not (B = .030). As you will be aware, the difference between a significant and a non-significant coefficient is not itself statistically significant unless directly tested. Please formally test the difference between the two cross-lagged paths (e.g., via an equality constraint and a chi-square difference test, with the estimate and confidence interval for the difference reported). If the difference is not statistically significant, the abstract’s claim that violence support predicted later obsessive passion but obsessive passion did not predict later violence support and the corresponding statements in the Discussion need to be softened accordingly; the syndrome framing does not depend on this asymmetry, and the paper is at its most credible where it does not lean on it.

In interpreting the many null findings in Study 2, it would be helpful to include a power sensitivity analysis that tells the reader what cross-lagged effect size you were powered to detect at the within-person level, given the sample size, attrition, and the limited within-person variance of a floored outcome. This bears directly on how much weight the non-significant forward path can carry.

A related point from our own review concerns estimation. In Study 1 you argue, convincingly, that the floored and skewed nature of these measures requires bootstrap-based inference, yet the Study 2 models rest on Wald intervals derived from FIML standard errors applied to the same kind of data. We understand that FIML precludes the case-resampling bootstrap, but robust (sandwich-type) standard errors and Bayesian estimation are readily available for RI-CLPMs and do not share this limitation. Please re-estimate the models with a robust or Bayesian approach.

To a reader who is not familiar with the reviews, the inclusion of identity fusion in Study 2 currently comes out of the blue. Please explain the rationale for including it in the introduction to Study 1. It is also somewhat unclear why its time-invariant score is not controlled for in the RI-CLPM alongside harmonious passion and commitment. Please control for it, at a minimum as a robustness test. As before, the outcome of these analyses will not influence our decision, but they need to be transparently reported.

In Study 1, you write: “Monte Carlo simulations indicated that N = 120 would provide power of .80.” Please specify for what effect this would provide sufficient power. The indirect effect? And of what effect size? Please also note that this power analysis concerns the between-person associations in Study 1 and does not speak to the within-person cross-lagged paths in Study 2, which is a further reason for the sensitivity analysis requested above.

The 4-factor model among Republicans in Study 1 obtained much better fit than the 3-factor solution, as you describe in your manuscript. Why then was the 3-factor solution used? We understand that this was for comparability, but it may undermine the validity of the results in that sample. More discussion of this limitation would be beneficial, including the trade-offs involved between imposing a common structure across psychometrically non-equivalent groups and modeling each group with its best-fitting structure. The best solution would be to utilize the four factor structure for this group.

In Figure 1, please provide 95% CIs and exact p-values after each coefficient. The figure is not very cluttered, so this should not be an issue. Alternatively, and maybe preferably, provide a table with this information (as you do in Study 2).

Make sure to export figures as SVG files (do not convert other formats into SVG, as this will not preserve the resolution) and enter them directly into the Word document.

We generally agree with the pivot toward the syndrome account, but we think the title needs to reflect it more. We suggest “The radicalization syndrome: Reciprocal dynamics of obsessive passion and deliberate engagement”, to foreground the novelty of your work more.

Relatedly, although you acknowledge in the Limitations that full measurement invariance across the ideological groups was not established, we encourage you to discuss this issue more explicitly when interpreting the Study 1 findings. Given that much of the manuscript emphasizes the consistency of patterns across groups, it would be helpful to clarify earlier in the manuscript what conclusions can and cannot be drawn in light of the lack of full measurement invariance, rather than leaving this point primarily to the limitations section.

Finally, the reference list needs attention. Please provide DOIs in URL format for every journal article. You can use our free tool at https://apa.advances.in to find DOIs automatically. The app will also help you spot some additional issues that need to be solved. Please make the changes and upload the file in the app again until you achieve a high score. Note that the app is probabilistic, so there will be occasional false positives, but these are generally few.

In summary, we believe the manuscript has improved substantially and that the remaining points, while requiring careful work, are well within reach. We would like to receive your revised manuscript, together with the response letter and the tracked-changes document, by August 15. Should you require additional time or wish to discuss how best to address these comments, please do not hesitate to contact us. We look forward to receiving your revised work.

Best regards,

Jonas R. Kunst & Milan Obaidi

Reviewer 1

See attached document. Thanks!

Reviewer 2

This revised manuscript applies several analyses that were requested, including an improved longitudinal modelling. However, there remain fundamental and unresolved problems at both the measurement and structural levels that prevent meaningful interpretation of the findings. First, the measurement models remain insufficiently validated. Across multiple constructs (obsessive passion, engagement, exposure subscales, support for violence), model fit is poor or inadequately reported, and discriminant validity is repeatedly unclear. In several cases, constructs that are central to the theoretical model (e.g., engagement vs. support for violence; obsessive vs. harmonious passion) appear not to be empirically separable. Second, the longitudinal structural models are built on unstable measurement foundations. RI-CLPM results are difficult to interpret given unclear factorial structure, measurement invariance, negative autoregressive effects, and insufficient reporting of key model components (e.g., random intercept correlations, residual covariances, and within-time structure). Third, there are serious concerns about model specification and interpretation, including: unexplained negative autoregressive effects, untested equality constraints across waves, unclear handling of within-person and between-person decomposition, and model fit that depends on inclusion of covariates. I elaborate on these points below.

Cross-sectional measurement analyses

1. Construct validity of obsessive passion

I remain unconvinced that obsessive passion is empirically distinguishable from harmonious passion, commitment, or closely related constructs in the extant literature.

The authors report a confirmatory factor analysis; however, the hypothesized three-factor model (obsessive passion, harmonious passion, commitment) shows poor fit according to conventional criteria (Hu & Bentler, 1999). In particular, RMSEA ≤ .06 and CFI ≥ .95 are typically considered indicative of good fit, which is not achieved here. Moreover, alternative two-factor and one-factor models also show inadequate fit, raising concerns about discriminant validity.

Given these results, it is difficult to conclude that obsessive passion is empirically separable from the other constructs included in the model.

In addition, if these constructs are measured within the same longitudinal dataset, it is unclear why longitudinal measurement models (e.g., longitudinal CFA and tests of measurement invariance) were not reported. Establishing factorial validity and invariance over time is a prerequisite for interpreting longitudinal structural effects.

2. Factorial structure of exposure subscales

The factorial structure of the exposure subscales remains unclear.

For the CFA models of active seeking, active action, passive online, and passive offline exposure, key fit indices (e.g., RMSEA, SRMR) are not reported in Table S2. Without these indices, it is not possible to evaluate model fit or the adequacy of the proposed measurement structure. This is a necessary prerequisite before substantive analyses or decisions to combine subscales.

In addition, the models reported in Table S3, including the merged constructs (deliberate engagement, incidental exposure) and support for violence, show poor fit according to conventional

criteria (Hu & Bentler, 1999). This raises concerns about the validity of the proposed higher-order structure and whether engagement can be meaningfully distinguished from support for violence.

Importantly, these measurement models do not appear to have been tested within a longitudinal framework. If adequate fit and longitudinal invariance were established, focusing on the structural model could be justified. However, this appears unlikely given that engagement and support for violence were not empirically distinguishable in the longitudinal analyses.

Longitudinal analyses

1. Discriminant validity and justification for separate RI-CLPMs

The manuscript reports that engagement and support for violence could not be empirically distinguished. Given this, it is unclear why separate RI-CLPMs are estimated for OP–engagement and OP–support for violence. If discriminant validity is insufficient, modelling them separately may not be justified.

The authors should clarify this decision and how results should be interpreted. If the constructs are not empirically separable, a single latent construct may be more appropriate.

More generally, longitudinal measurement validation (e.g., longitudinal CFA and measurement invariance testing) is needed to establish whether these constructs (as well as obsessive passion) are separable over time.

2. Random intercept structure and within-person decomposition

It is unclear whether correlations between random intercepts (OP with engagement/support for violence) were included in the models. These are central in RI-CLPMs for capturing stable between-person associations but are not shown in Figure 2.

The authors should clarify whether these correlations were estimated and report them. If omitted or constrained, this may bias interpretation of within-person effects by failing to fully separate stable between-person variance from within-person dynamics.

In addition, the authors should report (a) within-time residual correlations at T1 and (b) correlated residuals (or correlated changes) at T2 and T3, as these are important for interpreting contemporaneous within-person associations.

3. Negative autoregressive effects and interpretation

Within-person autoregressive paths are negative across constructs (OP ≈ −.36 to −.38; engagement ≈ −.47; violence ≈ −.23), but significance levels are not reported.

These negative coefficients are difficult to interpret substantively, as they imply reversal or oscillation of within-person deviations rather than temporal persistence. This raises concerns about potential misspecification or measurement issues.

The manuscript should provide a substantive explanation for this pattern, including whether it may reflect measurement properties, temporal spacing, or specification artefacts. Time invariance of the measures should also be tested and discussed in this context.

Given these dynamics, interpretation of cross-lagged effects is unclear, as they may reflect conditional processes embedded in an oscillatory system rather than straightforward directional influence.

4. Equality constraints and longitudinal invariance of structural paths

Cross-lagged and autoregressive parameters were constrained equal across waves, but it is unclear how this assumption of temporal invariance was evaluated.

The authors should clarify whether equality constraints were tested using appropriate model comparisons (e.g., likelihood-ratio tests, ΔCFI/ΔRMSEA). Without such tests, temporal stability of effects cannot be assumed.

More generally, it is unclear whether other longitudinal parameters (e.g., within-time residual correlations or correlated residuals) were also considered for equality constraints and how these decisions were justified.

5. Model-building sequence and transparency

The analytic strategy would benefit from a clearer and more transparent modelling sequence. It is difficult to follow progression from (a) measurement validation (reliability, factor structure, and ideally longitudinal invariance), to (b) unconstrained RI-CLPMs, to (c) testing equality constraints, and finally to (d) inclusion of covariates. Providing this sequence with full model comparisons would substantially improve transparency and interpretability.

6. Model fit dependence on covariates

A key concern is that RI-CLPMs for OP–engagement and OP–violence show inadequate or borderline fit unless covariates (harmonious passion and ideological commitment) are included.

In SEM, covariates are typically expected to explain variance rather than be required for acceptable global fit. This pattern suggests potential misspecification in the baseline model or unmodelled shared variance.

This is consistent with earlier concerns regarding measurement clarity. If constructs are not well differentiated, improved fit after adding conceptually related covariates may reflect absorption of unresolved construct overlap rather than substantive model improvement.

Conceptualization

Conceptualizing radicalization as a syndrome risks pathologizing a complex socio-political process and may reify a heterogeneous set of attitudes and behaviors into an assumed clinical-like syndrome without sufficient empirical or theoretical justification.

Round 3

Editor

Date: 2026-07-16

Dear Authors,

Thank you for the careful revisions. We believe that the paper now has solved the main lingering issues and can be accepted for publication in our special issue. Congratulations on an important contribution to the literature!

Best,

Jonas R. Kunst & Milan Obaidi

APPENDED DOCUMENT

Round 1 — Reviewer 1

Reviewer 1 review (Round 1).pdf

Review of: "Obsessive Passion and Political Violence: The Mediating Role of Exposure to Radical Content"

Thank you for the opportunity to review this manuscript. The paper addresses an important question—namely, why some individuals who are strongly committed to an ideology come to endorse political violence whereas many others do not (this is kind of the 'holy grail' of violent extremism research). I also think the attempt to distinguish between different forms of exposure to radical content is, in principle, worthwhile. That said, I ultimately do not think the manuscript delivers enough to merit publication. My main concerns: The paper's central mediator ("active action") is too close to the outcome domain to convincingly serve as an explanatory mechanism (and all variables seem to be extremely highly correlated, raising issues of discriminant validity), the causal language substantially exceeds what the designs can support, and the methodology, transparency, and presentation could be stronger. Additionally, the reliance on only MTurk samples, the unclear distinctiveness of obsessive passion as a construct in relation to nearby concepts such as fanaticism, and concerning statistical/analysis decisions are further issues. Taken together, these issues made me decide to decline the article. I know this will likely be disappointing, but I hope that my comments help you move forward (as I do think there's something there to build on!).

Please find my comments below. I hope you find them helpful and constructive.

Major Concerns:

1) I worry that active action and support for violence are conceptually too close to one another (i.e., border on the same thing). As described, active action includes behaviors such as meeting extremists face-to-face, attending protests, and searching for information about weapons or bombs. These behaviors reflect not just encountering radical content, but actively moving toward radical actors, settings, or violent practices (e.g., looking up ways to build a bomb—though I wonder how many MTurkers even said yes to such a thing; see issues regarding skew below). Generally, I think the paper suffers from a lack of clarity around the assessed constructs and ends up in a variable space quagmire that is difficult to understand/pick apart. This is especially so given that basically all the assessed variables correlate at > .6. This strongly suggests that the authors are assessing the 'same thing' (i.e., the same latent G construct) using multiple slightly different measures with different names.
2) The manuscript repeatedly frames the findings in causal terms, suggesting that OP channels people into radical environments, which then escalates violent intentions. But Study 1 is entirely cross-sectional, and mediation analyses in correlational data are a fairly big 'no-no' in my opinion. There is a large methodological literature warning against overinterpreting mediation in such settings (e.g., Bullock, Green, & Ha, 2010). Study 2 improves on this by using longitudinal data and latent growth modeling, but even here the evidence remains associational: changes in OP covary with changes in active action, which covary with changes in support for political violence. That is interesting, but it does not warrant the stronger claims made throughout the manuscript about dynamic causal

pathways. Moreover, in Study 2, I didn't see any test regarding directionality in the opposite direction or with the predictor and mediator flipped temporally (these are often conducted to show unique validity of the proposed temporal path; sorry if these were conducted and I missed them).

3) I was left unconvinced that obsessive passion, as used here, is clearly distinct from closely related constructs such as fanaticism, ideological obsession, identity fusion, or rigid ideological commitment. At points, the construct feels like a relabeling of already familiar ideas rather than a clearly differentiated theoretical contribution. The manuscript gestures toward passion theory, but it does not do enough to show why OP adds something unique to the study of radicalization beyond capturing intense or rigid ideological devotion (I recognize that ideological commitment is measured, but mere commitment isn't the same thing as rigid devotion, identity fusion, etc.). This matters because the paper's contribution depends heavily on OP being a meaningful and distinctive antecedent. As currently written, I am not sure the manuscript demonstrates that clearly enough. More work is needed to clarify what OP is, what it is not, and why it should be preferred over conceptually adjacent constructs. Perhaps a further study could include some of these other variables (e.g., identity fusion) and then compare their predictive power regarding varying 'exposers' and in turn support for violence? Generally, I think it's always good advice not to 'marry' a specific construct or scale and to assess a wide parameter space (when possible), in turn demonstrating conceptual replicability/discriminant validity.
4) Given that so much depends on how "active action" and the other EXPO-12 subdimensions are operationalized, the absence of item examples—or ideally a supplement containing the full measures—is frustrating. Readers need to be able to evaluate the operationalization directly.
5) The reliance on MTurk/CloudResearch samples is a major limitation. The manuscript makes broad claims about radicalization, extremist exposure, and support for political violence, yet the evidence comes largely from MTurk/CloudResearch self-report surveys. Many articles have demonstrated that MTurk is an awful data source. I'd suggest replicating the work using Prolific or a more reliable data source.
6) Regarding the data & statistics: It is unclear which statistical tests the authors conducted. For instance, in Study 1, are these linear regressions? If so, that's a problem given the massive, floored skew of most of the variables. These should generalized linear regressions accounting for skew (this is kind of bread and butter stuff when looking at variables like violent extremism given that they are always skewed).
7) Regarding the data & statistics: There are strong multi-collinearity issues that I think are impossible to resolve. Active seeking and active action correlate at around .8-.9 (Table 1). This basically means that any models including both variables suffer from multi-collinearity and won't provide reliable or stable estimates. **This is a major problem given that the authors' core contribution is that active action matters and the other exposure variables do not.** When looking at Table 1, this conclusion of the authors appears to be quite incorrect. Each of the other exposure variables predict political violence. It is only when they are entered into a multivariate model (I'm assuming this is what the authors did, but they don't describe the models conducted) that active action is 'uniquely' predictive. These models are likely to be inaccurate given multi-collinearity issues. This is a **severe** issue that compromises a core claim made in the article (to be honest, this is a mistake I see a lot of people make---meaning tossing highly correlated

variables into a multivariate model and then claiming that whatever survives matters while the other variables don't matter). Long story short, the authors' interpretation of the data is, as far as I can tell, inaccurate. The correct interpretation, according to the links shown in Table 1, that each of the exposure variables play quite a large role.

8) There also weren't any pre-registrations that I could find (I'm sorry if I missed them).

Thank you for giving me the opportunity to review your paper. Unfortunately, and as elaborated on above, I have numerous concerns that stand in the way of publication. I truly hope my comments are helpful and that my feedback is ultimately encouraging rather than discouraging.